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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of tropospheric ozone columns derived from assimilated GOME ozone profile observations" *by* A. T. J. de Laat et al.

Anonymous Referee #2

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Review of "Evaluation of tropospheric ozone columns derived from assimilated GOME ozone profile observations" by de Laat, van der A, and van Weele.

This manuscript describes the TORA method for producing tropospheric ozone column (TOC) estimates and presents results based on GOME ozone measurements from 1996-2001. This method uses a chemical transport model with analysed winds and parameterized ozone photochemistry to produce stratospheric ozone profiles that, when subtracted from a total ozone column observation, yield a residual tropospheric ozone column estimate. The chemical transport model is operated both in a "free running" mode and an assimilation mode whereby GOME ozone profile observations

are assimilated using a Kalman filter technique. Time series of TORA TOC values are compared with independent ozonesonde measurements over a 6-year period from 1996-2001 when GOME profiles are of sufficient quality. The authors report that assimilation of GOME ozone profiles improves both TORA TOC and UT/LS estimates in the tropics but degrades TORA performance in the mid-latitudes.

Overall the subject is appropriate for ACP. Tools for monitoring and analysis of global TOC are greatly needed, particularly as new remote sensing data sets become available. However, I have several concerns regarding the basic methodology, the presentation of results, and the conclusions drawn from these results that should be addressed before I can recommend publication in ACP. I have listed each of these below, followed by a list of minor comments and corrections.

First, the basic approach and methodology presented in this article both seem a bit outdated. Aren't there more sophisticated approaches available for assimilating global total ozone and profile measurements that have been shown to be quite effective (e.g., Schoeberl et al., 2007; Stajner et al., 2008)? What does TORA offer that will be better for monitoring TOC than other methods?

Second, it is very difficult to interpret Figures 2 and 4 due to their small size. I also found the labels and captions to be rather confusing, and the description of these figures lacking sufficient detail. Because of the way the information is plotted, it is difficult to see how the addition of the ozone profile information affects the TOC estimates. Based on the information in the tables and figures, it appears that assimilating the ozone profile information does not significantly improve global TOC estimates. Yet in the Discussion (section 5) the authors speculate that future applications of TORA using ozone profile measurements with a smaller footprint and higher horizontal resolution in the chemical transport/assimilation model should improve their results in mid-latitudes. Since the ozone profile information fails to add value to the TOC estimates outside of the tropics, in contrast to expectations, couldn't one also conclude that the assimilation system itself is fundamentally flawed? Based on the evidence presented here, it's not clear

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that increasing TORA model resolution to try and improve midlatitude performance is a worthwhile undertaking. The authors need to offer some quantitative calculations in support of their conclusion.

Third, it's not clear to me what the ultimate goal of this work is. Are the author's developing a system to analyze past global TOC behavior or are they interested in producing TOC analyses in near real time for monitoring purposes? If it's the former, the results seem to show that the use of an appropriate ozone climatology with analysed winds constrained by linearized chemistry is reasonable for estimating global TOC over the 1996-2001 period on monthly time scales. In this case, the meteorological analyses capture the vertical motions that drive tropospheric ozone fluctuations in response to synoptic variability in the upper troposphere. If it's the latter, Figure 5 appears to show that a more sophisticated ozone assimilation approach is needed (assuming that the TM4 results with full chemistry are more representative of the true annual mean TOC).

Specific Comments:

Abstract: In the last sentence, the authors state that the present results indicate that TORA residual should improve using MetOp/GOME-2 and EOS-AURA/OMI observations. Why exactly would this be so? Couldn't the present system be used to demonstrate this assertion using synthetic data?

Section 2: The paper would benefit from some general descriptive remarks about chemistry-transport models in general and the TM5 in particular. Why does TM5 employ a linearised ozone photochemistry parameterisation rather than full photochemistry? Can the authors describe in more detail the assimilation system? What are the main differences between the TM5 here and in Segers et al.? It was not clear at first what is being assimilated. I was under the impression that both total ozone measurements and profile measurements are being assimilated, but this does not appear to be the case. What are the relevant spatial scales for error propagation of the assimilated ozone profiles? Is the Fortuin and Kelder climatology used for the troposphere

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different than the climatology used in the Cariolle and Deque scheme? If so, are the climatologies merged in some way?

Section 3: I could not find Mijling et al 2009 in the reference list. Is this a published article? If not, it should be listed as a footnote following ACP style guidelines.

Section 4: I found the discussion in Section 4.l to be very confusing. According to the text, the UT/LS ozone columns from the free model simulation (i.e., no assimilation of ozone profile) are compared with selected sonde data in Figure 2. Yet the Figure 2 caption states this is a comparison of assimilated UT/LS columns with sondes. What ozone information is being assimilated in Figure 2? According to the figure legend, there is no assimilation. Figure 2 should be expanded into two four panel figures, one for the midlatitudes and one for the tropics. It is unreadable in its present form. Also, I could not locate station 394 in Figure 1. It would help the reader to place boxes around the selected 4 stations or otherwise highlight their locations in Figure 1. The apparent good agreement between the free model run and the monthly mean sonde data seems to reflect that the model chemistry relaxes the ozone back to climatology with $\sim$2 week time scale while the analysed winds capture the local fluctuations in tropospause height. Isn't the Fortuin and Kelder climatology is based on the same network of sondes that you are comparing against?

Section 4.2 could probably be merged with section 4.1 since it also deals with the free model run and finds essentially the same result.

The discussion in Section 4.3 focuses largely on temporal correlations. Is this the only relevant metric? Can you say whether or not spatial patterns improve with the use of the profile data? For example, does the bias arising from the use of a zonal mean climatology in the free model run in Figure 2 (lower left panel) improve when ozone profile measurements are assimilated?

Section 4.4 could also be merged with 4.3.

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What is lacking in Section 4.5 is a direct comparison of results between the free run and the run with assimilation. It is very difficult to ask the reader to examine two large sets of tables to determine the effect. Can you plot a sonde record along with results with and without profile assimilation (perhaps one for midlatitudes and one for tropics). Alternatively, the mean difference between the results with and without assimilation for each case, tropics and midlatitudes, could be plotted.

I don't understand Figure 5. Is the top panel supposed to be the "true" TOC distribution? Both the GDP and TOMS results show the high ozone over the equatorial Atlantic mentioned in the text. I don't see that in the TM4 result. Is this an improvement?

Section 5: Unless the authors provide additional evidence based on their model calculations, I don't see any support for the claim that increasing the resolution should improve the assimilation.

Minor corrections:

Page 11815, line 24: Meijer et al. 2006 missing from reference list.

Page 11833, line 23: Tarasick and Slater reference is out of order.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11811, 2009.